LETTERS TO THE EDITOR.

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The British Association and our Colonies.

PROF. MILNE'S letter in NATURE of November 23 (p. 77) will be read with pleasure by all scientific people in the colonies. The benefits to be derived from a colonial meeting such as he suggests are many and various, and I have no doubt will receive full consideration should the idea be given effect to.

Here in Africa, one piece of work which the British Association is the natural body to take up is that of the magnetic survey of the whole continent. The great lack of trustworthy data for immense tracts of this continent has been often commented on. When we remember how much of the continent is in British hands, and how the British Association since its inception has steadily helped and encouraged the study of earth magnetism, the fitness of bringing such a proposal before the association is apparent. At such a conference the possibility of a simultaneous magnetic survey of Australia would naturally be considered. Could these two surveys be carried out—even if very incompletely—they, with the surveys at present in progress and with the proposed ocean surveys of the Carnegie Institution, would form an invaluable contribution to our present knowledge of earth magnetism.

Africa has many other problems—educational, explorational, meteorological—the solution of which would be helped were they taken up at such a conference.

J. C. BEATTIE.

South African College, Cape Town, December 12, 1905.

Monotremes and Birds.

IN Semon's *Zoologische Forschungsreisen,* Lieferung xxii. (1904), Disselhorst treats of "Die maennlichen Geschlechtsorgane der monotremen und einiger Marsupialen." On p. 123 are two text figures, both copied from Sir Everard Home, *Phil. Trans.*, 1802, plate xii. One represents the male genital apparatus of *Echidna hystrix*, the other, Fig. 1B of the German work, the stretched male organ of the same animal. Now this Fig. B is, in Home's paper, correctly named "penis of the Drake." Needless to say, this Drake's organ does not in the least agree with that of Echidna, as which it is described in the German work, and our author is sorely puzzled about some of the details, *cf.* p. 131. This may well be the case. Errors and blunders have been made ere now, but the serio-comic point is that this Drake figure has, in the process of reproduction, assumed mammalian characters. In the original figure the base is surrounded by well-drawn feathers, and such are mentioned in the explanation of the plate. The thing is also correctly copied by Owen in his article Aves in Todd's "Cyclopædia," further in the "Anatomy of Vertebrates," vol. ii., Fig. 119, and Owen directs attention to an important error committed by Home in his interpretation of the urethra. But in the recent figure, wrongly attributed to Echidna, the feathers have lost their character as such, and have turned into hairy structures! Who has done this? The author or the artist, or have both combined to correct the faulty original? It is such a strikingly pretty figure that it is almost sure to be propagated, perhaps to be used as a proof of the affinity of the oviparous mammals. But a drake is a drake, for all that.

H. GADOW.

University Museum of Zoology, Cambridge, December 20.

Sounding Stones at Ch'üfu, Shantung.

LAST July I happened to pass through Ch'üfu, the birth- and burial-place of Confucius. In "seeing the sights" of the town I found three very fine examples of "sounding stones," or "stone gongs" as they are sometimes called. These particular examples do not seem to be very well known except by Chinese; none of my foreign acquaintances who have been in Ch'üfu had noticed them. Photo. No. 1 shows the tomb of the grandson of Confucius. The cover of the incense dish (on which my servant is resting his hand) is made from stone, but when struck with a stick, or even with the knuckles, it rings as though it were bronze. In fact, my man in the photograph refused to believe that it was anything but painted bronze until I myself assured him to the contrary. Photo. No. 2 shows two pillars (marked with crosses) of the balustrade in front of the principal hall of the great Confucian temple at Ch'üfu. Struck at any point with a piece of wood, they give a distinct musical note. Inside the temple is a large tablet, about $5 \times 3 \times \frac{1}{2}$ feet, of the same stone. In this case the note produced varies according to the point at which the stone is struck. The stone from which all these bodies is made is a greyish oolitic limestone. I was informed that it came from a quarry at Kwan Ko Shan, about seventeen miles south-east of Ch'üfu. Most of the stone from this place has no musical quality, but from time to time veins of it are found, and when found it is usually abundant. "Stone gongs" of this kind are found in all parts of the country, and some are in the possession of foreigners. So far as I can find out, they all come from this one locality. They have been known for many centuries, and it is recorded that the district from which they come paid its share of a certain special Imperial tax in "sounding stones." I should be pleased if any reader could give the cause of this very remarkable property, and if it is not understood I would gladly give what help I can towards elucidating it. During this journey I was pressed for time, and as my route lay directly east from Ch'üfu I was not able to visit the quarry. Should there be any object in doing so, however, I will take the first opportunity of returning and making any observations that I may be asked to make. Such an opportunity may occur at any time, and, in fact, could be easily made to occur, as the journey would only

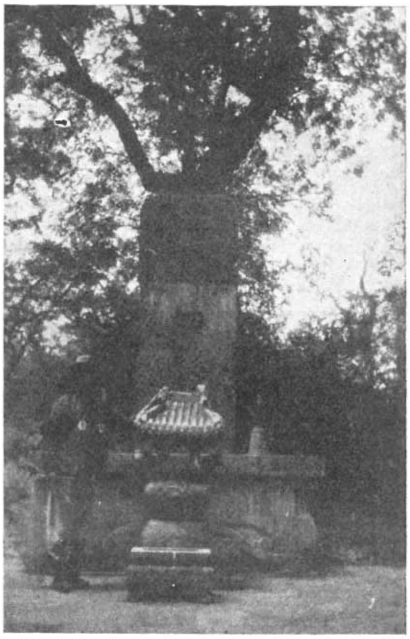

FIG. 1.—Grave of the grandson of Confucius. Cemetery of the K'ung family, Ch'üfu, Shantung.

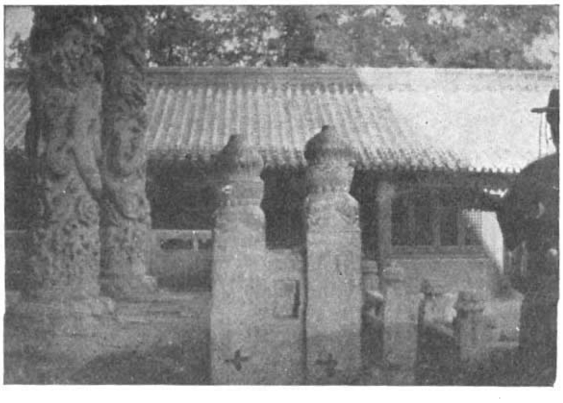

FIG. 2.—Detail of Confucian Temple, Ch'üfu, Shantung.

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take four days each way. I am afraid, however, that it might prove very difficult to secure any sample of this stone for transportation to Europe. ALFRED TINGLE.

Chinanfu, Shantung, China, November 9, 1905.

Auroræ of November 15 and December 12.

SINCE my communication of December 9 (NATURE, December 28), I have learned that the aurora borealis of November 15 was observed here by several persons between 8.30 p.m. and 9.30 p.m., Halifax time. The appearances were similar to those noted in England (NATURE, November 23, pp. 79–80), and the rosy-red streamers seem to have attracted special attention.

I am also informed that an aurora was observed here last night (December 12) at 9.30 p.m. with whitish streamers, but lacking the display of colour observed November 15.

It is somewhat noteworthy that the interval November 15 to December 12 covers a period of twenty-seven days—the time required for one complete rotation of the sun.

ALEXANDER GRAHAM BELL.

Beinn Bhreagh, near Baddeck, Nova Scotia,
December 13, 1905.

The Principles of Heredity.

I HAVE every reason to be satisfied with the kind and indulgent review (December 7, p. 121) by "A. D. D." of my book "The Principles of Heredity," but there is one sentence of it on which I should like to comment, more particularly as it contains nothing of blame or praise. "A. D. D." writes, "this book . . . is an embodiment of the recognition by medical men that they depend ultimately for a precise knowledge of nature on the professional biologist—who may or may not, at the same time, be a medical man."

But really I do not think that. On the contrary, I believe it is easily capable of demonstration that the information already in the hands of all medical men is incomparably superior, both in precision and volume, to anything ever possessed, or likely to be possessed, by biologists. It has not been utilised, that is all. The blame does not rest wholly with the medical man. His strictly professional curriculum is burdened by a monstrous but necessary load of facts. His one chance of coming in contact with subjects of general interest and of acquiring habits of sustained and accurate thought lies in the purely scientific part of his curriculum. Here his teachers are biologists who, instead of inculcating wide principles of heredity and evolution, add to the load on his memory by supplying irrelevant scraps of information about jelly-fish, earthworms, cockchafers, and the like—irrelevant, for, in the form they are presented, they do not link up with the studies and interests of his future career, and therefore are forgotten as soon as may be. "A. D. D." complains that I do not sufficiently appreciate classical teaching. It may console him to know that my appreciation of a certain class of scientific teaching is just as—well, hearty.

The biologist has surpassed the medical man in the study of great problems only because his attention has been directed to the subject, and because, on the whole, his habits of thought—not information—have been more precise. Had the medical man received the training of the biologist, or the biologist possessed the information common to medical men, the progress of science would have been much more rapid, and few or none of the great biological controversies of the past would have arisen, or at least have endured the interminable time they did; for example, the disputes as to whether natural selection is the cause or the sole cause of evolution, as to whether acquired characters are transmissible, as to whether variations are due to the direct action of the environment, as to whether evolution proceeds on lines of "fluctuating" variations or of discontinuous "mutations," as to the function of sex, and so forth.

Of necessity we—that is, all men—know the human type better than we can possibly know any other. Provided we know what to look for, extreme familiarity enables us to observe the smallest variations. No shepherd knows his flock, no biologist knows animal or plant as the medical man knows his fellow man. The species has diverged into a large number of natural varieties, dwelling under immensely diverse conditions and differing vastly in every peculiarity of body and mind. All these varieties, apparently, are inter-fertile, and almost all of them, in bulk or in isolated cases, have crossed with almost every other variety. Hybrids are being reared every day, and many races are compound hybrids—*e.g.* the Caucasian-Negro-Indian inhabitants of parts of South America. Above all, the species is being stringently selected and is undergoing rapid evolution under the action of disease, an agency which furnishes the most perfect series of experiments in heredity and evolution imaginable. Every race is resistant to every disease strictly in proportion to its past experience of it. Some diseases are short and sharp, others are of long duration. Some are local, others fill the whole system with micro-organisms or bathe the germ-cells with toxins. Many diseases are new to many races; others they have afflicted for thousands of years. If ever acquirements are transmitted, however "faintly and fitfully," it should be in the case of disease. If ever variations, no matter how small, are caused by the direct action of the environment, a race long afflicted should show the trace. If Mendelian phenomena play an important part in nature, we should note them in crossed varieties of men. If evolution proceeds on lines, not of fluctuating variations, but of stable mutations "which only selection can eliminate," then races (*e.g.* British) which have become highly resistant to this or that disease (*e.g.* consumption) should not constantly produce individuals who are as susceptible as members of a race which has undergone no such evolution (*e.g.* Red Indian).

Unless heredity in man differs from heredity in other species, it is very evident that medical men have no need to go to biologists for precise information, but that there is every need that biologists should go to medical men. A vast fund of minutely accurate data, much of which is statistical, is available. To grope in the obscurity that necessarily surrounds the past and the present of wild species or amid the confusion of the unrecorded crosses of domesticated varieties while this fund is untouched may be magnificent, but it is not science.

Southsea, December 11, 1905. G. ARCHDALL REID.

DR. REID takes exception to a passage in my review of his book; in it I state my belief that his book is the embodiment of a certain opinion, but Dr. Reid writes to say that he does not hold this view at all. It is not necessary, nor would it be profitable if it were, to discuss who is right in this matter—he or I—for obviously I am guilty of misrepresenting Dr. Reid's opinion.

But that the medical man is capable of acquiring a precise knowledge of nature independently of the information already gained and the methods employed by the biologist does not seem to me to be by any means certain. Dr. Reid thinks it is, and brings forward as evidence the fact that doctors possess better data for the solution of problems of evolution than ever have been, or can be, possessed by the biologist. Now, even supposing this to be true—which I do not for a moment—it does not seem to me to prove Dr. Reid's point. Either he thinks that the possession of data is tantamount to a precise knowledge of nature, or he does not; if he does, he proves his point by introducing into his syllogism a premiss which I believe to be untrue; if he does not—and I do not believe that he does—he does not prove his point.

But be this as it may, the point that interests me is that the belief that there is no great step between the collection of data and the derivation from them of a precise knowledge of nature is a widespread and, I believe, a profoundly erroneous one; for it seems to me that the possession of data is a small advance towards such a precise knowledge, and that that which hinders the acquisition of natural knowledge is not the slowness with which facts are accumulated, but the paucity of investigators capable of dealing with them properly; and this dearth is due to the infection of the majority of biologists by a disease—a sort of sleeping sickness—which consists in a disinclination to picture to the mind's eye the things represented by the words they use.

Let us proceed to examine Dr. Reid's main thesis—that

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